Dear Authors: Please respond to the report 2AR below. Many thanks.
Cordially, AVDM.

REPORT 2AR

Dear Prof. van der Merwe,

I do not want to stand in the way of publishing this paper especially if
there have been other positive referee reports and it is the wish of the
journal to do so. In general I think that the topic is interesting and
perhaps publishing it will further work on the problems the authors
address.

Having said this, I must confess that I'm not convinced by the authors'
responses to my report. Let me make a few remarks about the authors'
reply.

1. It still isn't clear to me that haven't begged several questions in
their rejection of Sklar's line on the spin echo results. They say, of the
coarse-graining approach, that its advocates will have to appeal to the
history of the system---a move that they (the coarse-grainers) do not have
to make in the case of other systems. Isn't it the case that the
interventionist must also explain this behavior? In the paper they say
that "the kind of thermodynamic behaviour we would like to explain using
statistical mechanics is the behaviour which leads to the usual situation
in which an innocent observer unaware of the history of the system will
actually make the *right* prediction, namely that the syustem is going to
stay in the equilibrium state for all future times." Is it the authors'
claim that statistical mechanics should not be expected to play a role in
the explanation of the innocent's surprise? How does the interventionist
explain the fact that the innocent makes the *wrong* prediction? Or, again
is that not something that can be/should be explainable by the
interventionist?

As I've said, I may very well be missing something here that is important,
but it does seem to me as if restricting SM explanations to "true
equilibriation" begs the question against Sklar who seem to think that the
observed anti-thermodynamic behavior is something SM can and should play a
role in explaining.

2. Looking back at the paper I see that the authors don't assert
explicitly that it is a virtue that their model has no stronger properties
than mixing, though it stills seems that the context of the remark is
naturally read in that way. The rejection of ergodic approaches as
candidates for explanation (because of KAM type results), however, seems to
me to be premature. These results may very well play a crucial explanatory
role even if the system may be slightly nonergodic.

3. Finally, I agree that there is an important difference between
restricting ones explananda to systems which are small subsystems of larger

systems/environments and the limitations due to features about
observational accuracy. On the other hand, I think I may be excused from
the criticism of discussing them in the same context given this sentence of
the authors' paper: "In the interventionist account, the emphasis is
shifted from a limited measurement resolution towards measurements which
are restricted to limited, interacting systems."

omit or p.17

My main worry here was and is that there is a fairly standard line against
interventionist approaches which the authors seem to ignore; namely, that
one can always "expand the 'system'" to include part of the larger
environment. Then the interventionist line with respect to the original
"system" won't seem to apply (or so the argument goes). In effect this is
why I asked what justifies the delimitation of the system as the 'system'
to be investigated.

Dear Michael,

here is a draft of a reply to our referee. I will make some small changes
to the paper, in response to the points the referee raised - as soon as I
have a new version ready I will send it to London.

best wishes,
katinka

Dear Professor Van der Merwe,

thank you for sending us the second referee's report concerning our paper
"The Spin-Echo Experiments and the Second Law of Thermodynamics". In
response to this report we would like to raise the following points.
First of all, the referee asks for our motivation for developing our model
of the spin-echo experiments. In the literature on the foundations of
statistical mechanics, the spin-echo experiments are often quoted as an
example of apparent anti-thermodynamic behaviour. These experiments thus
play a crucial role in the debate about the appropriate statistical
mechanical analogues of equilibrium and entropy. We thought it appropriate
to give a complete mathematical account of a simple model of the spin-echo
experiments, so as to bring out the conceptual issues in the clearest
possible way. As far as we are aware, this model has not been treated
elsewhere in the physics literature in such explicit detail. We believe
that statistical mechanics is concerned with explaining the approach to
true equilibrium, not quasi-equilibrium, as the appropriate analogue for
thermodynamic behaviour. Otherwise the spin-echo experiments could be
regarded as exhibiting anti-thermodynamic behaviour, which we do not
believe to be the case. Our discussion of the "innocent observer" is
meant to bring out the problems the advocates of the coarse-graining
approach face. It is a short-coming of the coarse graining approach that
in this approach the distinction between quasi-equilibrium and true
equilibrium cannot be made. In order to avoid being forced to say that the
spin-echo systems exhibit anti-thermodynamic behaviour, they will have to
appeal to the history of the system, a move they do not make in the case
of other systems.

Second, we never claim that it is a virtue of our model that it is a
mixing system and has no stronger ergodic properties. On the contrary, our
remark about the infinite times the system needs in order to reach the
equilibrium state is meant to point at the problematic aspects of
approaches based on mixing properties, since we are convinced that
statistical mechanics should reproduce the finite relaxation times we find
in real thermodynamic systems. The interventionist approach we defend in
this paper makes no reference to ergodic theorems, and may be expected to
produce more realistic relaxation times to true equilibrium even for
mixing systems. In general we reject ergodic approaches, since they do not
appear to be relevant for realistic systems (cp KAM theorems, etc.)

Third, the distinction between a "small" system and a "large" environment
seems to us a perfectly intelligible and objective one, and is quite
unlike the coarse graining appeal to limitations of observation.
Restricting one's ambitions to explaining the behaviour of small, limited
systems in terms of the properties of these systems and their environments
is not the same as explaining the behaviour of isolated systems in terms
of our limited abilities to measure the properties of these isolated
systems.

Our reference to those authors who claim an increase for the entropy of
the whole universe is intended merely to say how they get their results,
not at all to say we agree with them.

Finally, we are grateful for the reference to Prigogine's paper, which we
were not aware of, and which we will include in the paper.

Regards,

Prof. M.L.G. Redhead and T.M. Ridderbos